THE ANALYST.

MAY, 1893.

ANNOUNCEMENT.

A World's Congress of Chemists will be held at Chicago, U.S.A., during the latter part of August, 1893. The exact date is not yet fixed. The entire field of chemistry will be covered, ten sections having been constituted. The department of *Sanitary Chemistry* has been assigned to Dr. Henry Leffmann, 715, Walnut Street, Philadelphia, for organization, and he will be pleased to receive notice of any papers intended to be read in that department, or to furnish information to those intending to contribute to the work of the section.

PROCEEDINGS OF THE SOCIETY OF PUBLIC ANALYSTS.

The usual Monthly Meeting of this Society was held on the 5th April, at the rooms of the Chemical Society, Burlington House. In the absence of the President, Mr. Alfred H. Allen occupied the chair.

The minutes of the last meeting were read and confirmed.

The following gentlemen were proposed:—As Members, Mr. William James Orsman, F.C.S., F.I.C., Public Analyst, Wigan; Mr. Charles E. Sohn, F.I.C., F.C.S., Anerley. As Associates, Mr. A. H. M. Muter, assistant to Dr. Muter; Mr. C. Hubert Pope, assistant to Mr. Horace Sweete.

The following gentlemen were duly elected:—As Member of the Society, Mr. W. C. Williams, B.Sc., Liverpool. As Associate, Mr. H. I. Foster, Hull.

The following paper was then read by its author:—

THE LISTER-BABCOCK MILK TESTER; WITH SOME SUGGESTIONS FOR EXTENDING ITS USE.

By G. Embrey.

After the very large number of papers which have at different times been brought before the notice of this Society relating to milk analysis, it would seem that the subject had become exhausted; and the splendid process of Dr. Adams is so near perfection, that it would appear nothing is left to improve, yet analysts rightly attach much importance to simple, yet fairly accurate, methods as a means of sorting samples. The most promising of these would appear to be those depending on the solution of the curd, and setting free the fat in a convenient form for measurement. Any method which is entirely volumetric and dispenses with weighing will always command attention.

Dr. Paul Vieth (in Analyst, vol. xiv., page 86) has given a good summary of the various methods of milk analysis; I will therefore confine myself to those in which centrifugal motion is employed.

The first, and probably one of the best, is the Lactocrite, the merits of which have been brought before the members by my friend, Mr. Faber, and there is no doubt it is a very reliable and accurate instrument.

One objection, and a fatal one, to its use in a dairy, or even in a laboratory in which a balance is kept, is the need of heating with a volatile acid and the enormous speed required—6,000 revolutions per minute.

In reference to the accuracy of this instrument, Dr. Vieth remarks:—"It is generally admitted that results thus obtained agree very closely indeed with those arrived at by determining the fat gravimetrically. At the same time, it is rather curious that some experimenters—*e.g.*, W. Blyth—compare it with Adams' method, while others, among them Soxhlet, compare it with the latter's method, both parties find agreeing results, and still paper and plaster methods do not give results agreeing among themselves."

I am not suggesting, nor I am sure did Dr. Vieth, that either of these gentlemen were wrong or had suited their figures to their desire, for I think this discrepancy may be explained easily. In processes of this kind different methods of working will produce different results, as I shall show, and in determining the value of a new method it is needful to have a standard; we then work our new process until the results reach our standard. For myself I take Dr. Adams' paper coil method, and alter the method of working in a new process till I reach my ideal. I think it will be generally, if not universally, admitted that Dr. Adams has given us the best method of determining fat in milk.

The other objection to the use of the Lactocrite is cost.

The next process I would refer to is that of Leffmann and Beam, whose apparatus is less costly than the Lactocrite, still, it is twice as much as the Lister-Babcock. Another serious objection to their apparatus is that the substance measured is not fat, but fat and fusel oil. It is also objectionable in consequence of the acid fumes.

In vol. xvii., page 127, THE ANALYST, will be found a paper on the Babcock process, by Mr. Stokes, in which the method of using the apparatus is fairly well described, but from his description of the condition of fat and the poor results obtained, there is no doubt that he adopted altogether erroneous methods, for which he is in no way responsible, as he followed the instructions sent with machine.

The instructions generally sent out with the apparatus (which are copied from the American pamphlet) direct that sulphuric acid of sp. gr. 1·843 should be used. This is the gravity in the British Pharmacopœa for acid containing 98 per cent. of the true acid, but if one asks a Pharmaceutical Chemist for a sample of pure sulphuric acid, it will have a specific gravity of 1·840 or 1·841, which, according to Otto's table, contains 98 per cent. I have no doubt that this is the reason why Mr. Stokes obtained such poor results, the success of the operation depending almost entirely on employing acid of proper strength. If too strong, the curd is charred and it is impossible to read the fat correctly; if too weak, the curd will not all be dissolved. After a considerable number of experiments, I have fixed as limits sp. gr. 1·831 to 1·834. Acid of this strength will be produced by taking nine parts of B. P. acid and one part of water. So important is this strength of acid that I have had constructed a hydrometer, with two lines marking the gravity, so that in using it it is only necessary to dilute the strong acid till the liquid surface lies between them when the mixture is cooled to 60° F.

Corrected instructions for using the Babcock tester.

Measure the milk by pipette into the bottle, holding the bottle in an inclined position. Measure out the quantity of acid (17·5 c.c.) in the measuring glass, and pour into test bottle. In pouring in the acid the bottle must be rotated in order to wash all the milk down from its neck, otherwise a clot of undissolved curd may form in the fat. Well mix the acid and milk and whirl in machine for ten minutes, giving about 60 turns of the handle per minute. Fill the bottles up to the 7 mark with water which has just ceased boiling; put some of this same water into the tank of machine: again whirl the bottles for two minutes, and measure the fat.

The first form of apparatus was well described by Mr. Stokes, THE ANALYST, vol. xvii., p. 127. The most serious fault in the original was the oscillation when full speed was obtained. It was certainly a clumsy piece of apparatus. Messrs. Lister have, I think, succeeded in producing a much better machine and this I have the pleasure of showing. It will be seen that it is compact and entirely overcomes the difficulties of the original form.

In examining the results and comparing with coil extraction and calculations from Hehner and Richmond's tables, we get what appears a serious discrepancy.

Fat by Babcock.	Babcock + ·3.	Fat by Adams.	Babcock + ·4.	Calculated Fat.
2·5	2·8	2·83	2·9	2·88
3·1	3·4	3·39	3·5	3·47
3·1	3·4	3·42	3·5	3·48
3·3	3·6	3·62	3·7	3·66
2·9	3·2	3·24	3·3	3·31
3·0	3·3	3·3	3·4	3·36
3·2	3·5	3·48	3·6	3·57
1·4	1·7	1·67	1·8	1·75
4·1	4·4	4·42	4·5	4·5
2·7	3·0	3·05	3·1	3·09

It will be seen that we obtain a result ·3 per cent. below Adams' coil and ·4 per cent. below calculated fat, and my experience has always been that fats calculated according to these tables are generally higher than those obtained by coil extraction. This was referred to by Mr. Stokes, THE ANALYST, vol. xiv., p. 30.

No. 1	...	2·91	Adams	3·14 Calculation.
„ 2	...	2·71	„	3·11 „
„ 3	...	2·48	„	2·95 „

The question now arises, why this difference? Mr. Stokes suggests fat particles retained by white sediment in bottle or locked up in black masses of charred curd.

I separated from several bottles the white sediment, and examined with microscope, but failed to find fat globules, the mass consisting of thin plates of selenite, which show good colours when placed betweed Nicol prisms. As regards the charred particles referred to by Mr. Stokes, these need not exist if the apparatus be properly worked.

How then can we account for the deficiency? This is simply due to inaccuracy in graduating the tubes; of course, like all constant errors, it is of no consequence and can easily be allowed for.

The advantages I claim for the Babcock over its predecessors are :—

1. No volatile acid required.

2. No artificial heating.

3. Pure fat alone measured.

4. Cheapness.

5. Small number of revolutions per minute required as compared with the Lactocrite (600 against 6,000).

6. Fat may be measured many days after an experiment by warming and whirling again.

It occurred to me some time since to use the tables of Messrs. Hehner and Richmond for calculating the total solids, but I find these gentlemen had forestalled me. I have, however, made a correction on the lines indicated, viz., reducing by ·1 per cent., and carrying the results only to one place of decimals, and in this way furnished agriculturists with a simple method of determining the value of milk for cheese-making. This with the special table of Dr. Babcock for ascertaining the amount of butter available, gives all that is required by the dairy farmer, and also as a sorting method for the analyst. The limit of error I fix at ·1 per cent.

I do not suggest that in the case of adulterated samples it should be used, except as a check; the elegant method of Dr. Adams should in these cases be employed.

I give the following table as showing how the results work out in practice:—

	Samples A.	B.	C.
Total solids	12·24	9·26	12·55
Specific gravity	1032·88	1024·8	1032·8
Fat by Adams	3·28	2·52	3·45
„ calculation	3·34	—	3·61
„ Babcock	2·9	2·2	3·2
Solids-not-fat from Adams	8·96	6·74	9·35
„ „ „ Babcock (·4 added to fat) ...	8·94	6·66	8·95

Sample B. was obtained by taking 150 c.c. of A. and adding 50 c.c. of water. The figures obtained bear this out.

Example of solids calculated from Babcock test:—

Sp. gr.	Fat by Babcock.	+·4	Solids from Babcock.	Solids estimated.
1024·9	3·0	3·4	10·3	10·25
1033·68	1·8	2·2	11·06	10·97
1024·6	3·0	3·4	10·16	10·02

Description of Apparatus.

Fig. 1. Original Babcock Machine. In this machine the bottles AA are fitted in tin tubes on periphery of rotating wheel. The speed is obtained by pressure of large wheel B on india-rubber pulley at C.

The chief faults are:—

1st. Centre of gravity of rotating part is too high, causing great oscillation at high speed.

2nd. The india-rubber pulley soon becomes softened.

Fig. 2. Lister-Babcock machine. This has the following advantages:—compactness, no perceptible oscillation at whatever speed it may be used; the bottles are fitted in wire cages so that a better position is secured while whirling, and when at rest they become vertical.

The speed is obtained by a toothed pin A, working inside larger toothed wheel B, the direction of motion being changed by a pair of bevel wheels C. The bottles E E hang in steel wire cages resting in slots in cast iron flange D.

Fig. 1.

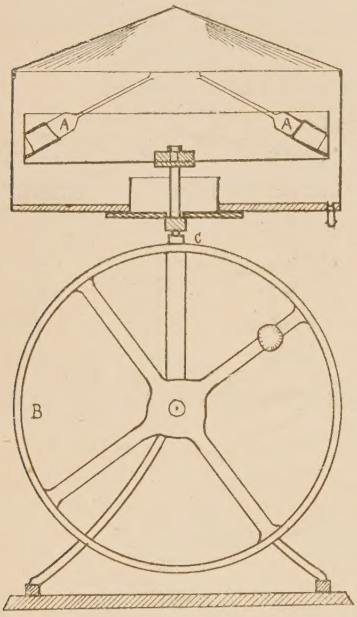


Fig 3

Fig 4

Fig 2.

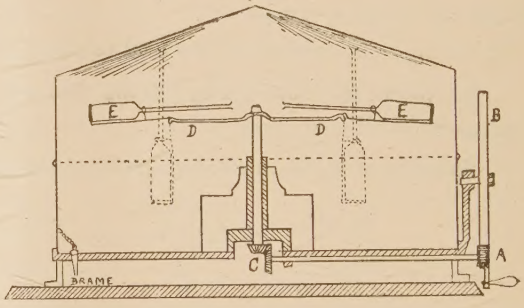


Fig. 3. One of the test bottles.

Fig. 4. Special hydrometer with one mark at 1·831 and another at 1·834 for determining strength of acid.

I must express my indebtedness to Mr. J. S. S. Brame for the drawings which accompany this paper, and also for much help in working out the most suitable conditions for obtaining reliable results. I have also to thank Messrs. Lister & Co. for the loan of apparatus and ready help on all occasions.

Since the above paper was written I have had an opportunity, by the courtesy of Mr. Droop Richmond, of more carefully examining the Leffman-Beam apparatus, and I certainly think if this be used some steps should be taken to protect the persons using it. If the pins holding bottle sockets get broken while in use (a very likely thing) the enormous speed would be almost certain to drive the liberated cup through the operator's body.

I have also learned that the bottles used in the Babcock process are calibrated, so as to give results enabling agriculturists to determine the amount of fat available for butter-making and not the actual amount in the milk.

Discussion.

Mr. Allen said that the apparatus was undoubtedly an interesting one, and one which all Analysts would be anxious to experiment with. Mr. Embrey seemed to have overcome several important difficulties. He (Mr. Allen) had been prevented from using the Leffmann-Beam centrifugal method, because he had felt that he did not know exactly what he had been measuring. To be able to get the pure fat out was a distinct advantage, and it would seem that a practicable method had been brought forward. He was glad to think that Mr. Embrey had found a satisfactory explanation of the discrepancies between this method and the Adams' coil process.

Mr. Embrey stated that the apparatus had been patented by Messrs. Lister & Co., Limited, of Dursley, England, and that the price of an apparatus fitted with eight tubes was £2 10s.

Mr. Allen pointed out that Thörner, who had done much work with this apparatus, had adapted centrifugal force to several other analytical processes, employed 6,000 revolutions per minute. He had been thinking of using the method for obtaining the unsaponifiable matters from oil. Mr. Embrey had laid great stress upon the strength of the acid in milk analysis, but it seemed to him that the difference between 1·843 and 1·840 was very small. He understood Mr. Embrey to say that the acid he used was made by mixing nine parts of B.P. acid with one part of water.

Mr. Embrey pointed out that when sulphuric acid and water were used, contraction took place. He had determined the exact strength of the acid by precipitation after diluting.

Mr. H. Droop Richmond said that the density of strong sulphuric acid (*e.g.*, 99 per cent.) was less than the density of sulphuric acid when it was diluted with a small quantity of water, the maximum density being reached at 97·5 per cent.*

Sulphuric acid was in this respect analogous to the well-known case of acetic acid, but the change in the density curve was less marked.

He had examined the machine before the meeting, and it seemed a good and ingenious one. He was in the habit of using the Leffmann-Beam method. With reference to Mr.

* The following figures, taken from Mr. Richmond's tables of the specific gravity of sulphuric acid (*Jour. Soc. Chem. Ind.* ix., 1890, No. 5), show the change in density :—

Sp. Gr.	Strength.	Sp. Gr.	Strength.
1·840	99·61	1·843	96·93
1·841	99·29	1·841	95·40
1·842	98·89	1·840	94·96
1·843	98·08	1·834	92·98
1·8432 (Max.) ...	97·50	1·831	92·22

Embrey's statement that it had been proved by Mr. Hehner that a mixture of fat and fusel oil was measured in the latter method, he was conversant with Mr. Hehner's paper, and it only contained a suggestion in that direction. It was he (Mr. Richmond) who had brought forward evidence in proof, but further investigation showed that this view was erroneous. He had, therefore, withdrawn it, and had entered into detail in his second paper on the Leffmann-Beam process. He was of opinion that the Leffmann-Beam process possessed advantages over the Lister-Babcock process, since instead of turning for ten minutes and stopping to pour in hot water, it was only necessary to turn for one minute. As for the acid fumes flying about, he had not observed or been incommoded by them. He had himself obtained a couple of results on a two-bottle machine under two minutes. It was very apparent that with the other process it would take a quarter of an hour to obtain a result. He had not found that the bottles of the Leffmann-Beam machine broke, even when filled quite to the top; in fact, he always filled his bottles within a quarter of an inch of the top, and always obtained satisfactory results. With the Leffmann-Beam machine it was possible to read to rather less than 0·1 per cent. He always read to a quarter of a division, which equalled 0·025 per cent. It might be said that it was a refinement to do that, but the delicacy was increased. He had published the figures of many experiments showing the great concordance. One other advantage of the Leffmann-Beam method was that there was no necessity to trouble about the strength of the acid; it could be taken as it came and used straight away. The variation in the strength of the acid made very little difference. For some time he had been investigating the estimation of total solids, and he had now come to the conclusion that the determination of the total solids was one of the least accurate estimations in milk analysis. He had found out that according to the way in which the experiment was carried out, the results could be easily varied within a quarter of a per cent., *practical* constancy being always obtained. He had made a considerable number of experiments with the asbestos method of total solids estimation, and had found that the calculated result was usually about 0·1 higher than the fat as estimated. He believed the reason for this was because in the asbestos method the total solids obtained were higher. The average total solids by the S.P.A. method were a trifle too low, and as the formula was calculated for this method, it would naturally not agree with a method yielding different results. Mr. Embrey had given Mr. Hehner and himself (Mr. Richmond) the credit of being the first to calculate the total solids from the fat and the specific gravity; but they had only modified a formula which had been used before them by Fleischmann and Morgen.

Mr. Jones said that he had not seen the machine before. It was well known that the difference in the results obtained by the Adams process and by the old method of extraction from the solids was about 0·4. That had been accepted. The cause of the discrepancy was also well known, viz., that the fat was retained in the solids-not-fat. He could not explain the difference in this particular case. But if it was due to graduation,

he thought it could be got over satisfactorily. It was easy to rectify the graduation of the tubes, say by taking a larger quantity of milk.

Mr. Lister explained that the time mentioned for an experiment, viz., ten minutes, came from America. He had tested the Lister-Babcock machine and had obtained exactly similar results, whether he had turned for ten or for two or three minutes. The ten minutes rule had been strictly followed out here, perhaps because it had come from America.

Mr. Cassal said that at the time the paper, which had been alluded to on centrifugal separations, had appeared, he was himself engaged in making experiments in that direction, and the few results which had been obtained by Mr. Gerrans and himself, up to the time of publication of the paper referred to, had led them to hope that there was an important future for the centrifugal system of separating mixtures of liquids, and mixtures of liquids with solids, as in the case of precipitates. There was much to be done in that direction. In this respect he thought the machine before them might be very valuable.

Mr. Woosnam said that he believed it had been stated by Mr. Hehner that the red colours seen in the mixtures in the Babcock test-glasses was always obtained when an acid acted upon fats in the presence of a carbohydrate. He (Mr. Woosnam) was inclined to think, therefore, that the low results obtained by the Babcock process were due to loss of fat from the action of the strong acid, rather than to errors in graduation. He did not believe that this was a machine which analysts would take up for the purpose of testing milk (although they might possibly do so for throwing down precipitates) because there were other processes which were far more reliable and accurate. He considered that for dairymen, &c., the use of strong sulphuric acid was dangerous, and the machine for this reason unsuitable.

Dr. W. J. Sykes called attention to a recent extension of the use of the centrifugal machine. A paper had been read a short time since by Dr. Croll showing that it was possible in this way to entirely separate yeast and other matters suspended in beer, leaving the latter in an absolutely brilliant condition. The author of the paper thought that this might even be effected on a commercial scale.

Mr. J. Baynes then exhibited an apparatus for distilling off the ammonia in the Kjeldahl process for the estimation of nitrogen in organic substances. With this the proceedings of the Society terminated.

ON A SOURCE OF ERROR IN THE VOLUMETRIC ESTIMATION OF CHLORIDES BY MOHR'S METHOD.

By W. Gathorne Young.

(*Read at the Meeting, March 1st*, 1893.)

The reliability of Mohr's method for the volumetric estimation of chlorides by titration with silver nitrate, in which chromic acid, or rather a neutral chromate, is used as an

indicator, has been questioned by Biscaro, whose observations have been confirmed by Forbes-Carpenter. These investigators attribute the error to the solubility of the neutral silver chromate in a solution containing nitrates, especially those of the alkalies. My father also has pointed out, in a paper read before this Society at one of its first meetings, the inaccuracy of this process when certain phosphates are present. During some experiments on chlorides I was led to believe that, although the presence of nitrates in *large* quantities may greatly influence the result, the most important considerations are the temperature and volume of the water present.

I therefore made several experiments on known quantities of NaCl dissolved in water at different temperatures and volume, the results of which show that in order to obtain correct estimations by this method, it is necessary to keep the volume of the solution very low, and to operate upon the smallest amount of chloride consistent with accuracy, in order that the volume of silver nitrate solution to be added may be small, as the water introduced in this way interferes considerably with an accurate determination as to the end of the reaction.

The sodium chloride solution used was made by dissolving 1 gramme of the pure re-crystallised salt in 250 c.c. of distilled water at 15·5°C. The silver nitrate was prepared from fresh crystals of the perfectly pure and dry salt by dissolving 4·79 grammes in 1 litre of water at 15·5°C., 1 c.c. of this solution being therefore equivalent to ·001 gramme chlorine.

The first six experiments were made on 5 c.c. of the NaCl solution, that is ·02 grm. NaCl; they were conducted at the normal temperature 15·5 deg. C. The results are as follows:—

First Series.

1.	5 c.c. NaCl solution evaporated to dryness required		12·2 c.c.
2.	5 c.c. „ „ „ as before „		12·2 c.c.
3.	5 c.c. „ „ titrated without evaporation required ...		12·2 c.c.
4.	5 c.c. „ „ made up to 25 c.c. with water at 15·5° C. required		12·3 c.c.
5.	5 c.c. „ „ „ „ 50 c.c. required		12·4 c.c.
6.	5 c.c. „ „ „ „ 250 c.c. „		12·8 c.c.

As the volume of $AgNO_3$ solution which should be required for ·02 grm. NaCl is 12·14 c.c., the error in the last experiment amounts to nearly 5 per cent.

From these results it will be seen that the most accurate determinations were obtained when the NaCl solution was evaporated to dryness and titrated in that condition, and that the error increases as the excess of water.

In the second series of experiments, 25 c.c. of the NaCl solution, which contains ·1 grm. NaCl, were used, as it was thought advisable to try the effect on the result of larger quantity of the salt.

Second Series.

1. 25 c.c. evaporated to dryness and titrated, required ... ... 61 c.c.
2. 25 c.c. evaporated to a small bulk (about 5 c.c.) required ... ... 61 c.c.
3. 25 c.c. evaporated to dryness and dissolved in 25 c.c. water, required 61·2 c.c.
4. 25 c.c. at 15·5° C., titrated without evaporation, required ... 61·2 c.c.
5. 25 c.c. made up to 250 c.c. with water at 15·5° C., required ... 62 c.c.

25 c.c. of the NaCl solution should require 60·7 c.c. $AgNO_3$ solution, so the same fact will be observed from these results, namely, that the error increases with dilution. It is interesting that although the amount of sodium chloride used, and therefore of sodium nitrate in the solution, was five times as much as in the first series, the nitrate present had no effect on the result.

An experiment was then made on 250 c.c. of pure distilled water, free from NH_3. This was titrated with the standard silver solution. There was no perceptible change in colour until ·8 c.c. had been added, though, of course, the water was absolutely free from chlorides.

Two experiments were then made at 38° C. on 5 c.c. of the sodium chloride solution. In actual practice determinations are often made at about this temperature, as it is customary to wash residues, etc., with fairly warm water, in order to extract chlorides, and to titrate without cooling.

Third Series.

1. 5 c.c. NaCl solution made up to 100 c.c. at 38° C., required ... 12·6 c.c.
2. 5 c.c. „ „ „ 250 c.c. „ „ ... 13 c.c.

The volume of $AgNO_3$ solution, which should have been used, is 12·14 c.c.

From these results it will be seen that if it be necessary to dissolve the chloride to be estimated in even moderately warm water, it is very important that the solution be cooled before titration, as the presence of heat increases the error, and that in no case should the titration be performed on a very hot or boiling solution, as will be seen from the following experiments.

Fourth Series.

1. 5 c.c. NaCl solution made up to 100 c.c.; titrated at 100° C. required 13·1 c.c.
2. 5 c.c. „ „ „ „ 250 c.c.; „ 100° C. „ 14·8 c.c.

In both cases the volume of $AgNO_3$ solution taken should have been 12·14 c.c.; the error, therefore, in the last experiment is no less than 17 per cent.

As all these results tend to show that the error is caused by the silver chromate dissolving in the excess of water present, it was thought advisable to determine its solubility.

Some neutral silver chromate was prepared by decomposing potassium chromate with silver nitrate. The resulting precipitate was washed with boiling water repeatedly and thoroughly. It was then dried. About 2 grammes of this were, in order to effectually free the salt from any impurity, digested with boiling water for about 12 hours, more water being added from time to time as the solution evaporated. The undissolved chromate was allowed to settle down and the clear solution decanted. The chromate was again washed by decantation, about 300 c.c. of water added, and the whole digested on a sand-bath until a saturated solution was obtained; it was then set by for a day. The solution was then decanted, cooled to 15·5° C., and the solubility of the chromate at that temperature ascertained by evaporating 100 c.c. to dryness in a carefully weighed platinum basin.

It was found that 100,000 parts of water at 15·5° C. dissolve 6 parts of chromate.

The solubility in boiling water was then determined with the result that 100,000 parts of boiling water dissolve 18 parts of chromate.

These experiments show that fairly accurate results may be obtained by Mohr's process, if the solution be kept down to a small bulk and at a low temperature.

This is rather important, as in many cases, more especially in water analysis, it is usual to take a certain volume of the solution for examination and to estimate the chlorides without previous concentration. The error thus obtained is considerable; as, in the drinking water supplied to London, the solution of chlorides must be very dilute as the chlorine seldom exceeds ·02 grammes per litre.

In order to show the importance of these errors in the case of water analysis, I made the following experiments on East London water:—

Fifth Series.

1. 100 c.c. evaporated to dryness and dissolved in about 2 c.c. water, gave 1·26 grains Cl per gallon.
2. 100 c.c. titrated at once, cold, gave 1·47 grains Cl per gallon.
3. 100 c.c. „ 38° C. „ 1·61 „ „
4. 100 c.c. „ 100° C. „ 1·82 „ „

These experiments were repeated with precisely similar results.

In the last case the error is nearly 31 per cent. In the experiments in which the water was first evaporated and then dissolved in a small quantity of water, the titrations were performed in the platinum basin, in which the evaporation was made.

It may be mentioned that in the experiments on dilute heated solutions, though the error shown is very great, it would be even greater, had the appearance of a permanent red precipitate been taken as indicating the end of the reaction.

In these cases the change of colour was so very gradual that I found it extremely difficult to decide unless the solution were compared with a very dilute solution of potassium chromate, and the slightest change in tint taken as the end.

If after this more silver nitrate be added, no precipitate appears for some time, the only change being a deeper red colour to the solution owing to neutral silver chromate $Ag_2 CrO_4$ going into solution.

In estimating chlorides with standard silver nitrate, therefore, it is advisable to evaporate to dryness before titration and weak solutions of silver nitrate are to be condemned when working upon anything but very small quantities of chloride, on account of the dilution which they cause.

Discussion.

The Chairman welcomed Mr. W. G. Young as the son of an old and valued member of the Society, and invited discussion on the Paper just read.

Mr. R. Bodmer presumed that the chromate of potassium used in the experiments was free from chlorides. It was a good plan to have by the side of the basin in which the determination was made a similar basin containing pure distilled water coloured with the same amount of the chromate solution. The reading was thereby rendered much finer. The extreme differences recorded by Mr. Young with the same solution were certainly surprising.

Mr. H. Droop Richmond asked Mr. Young if he had noticed in The Analyst* three or four years ago, a paper abstracted from the *American Chemical Journal*, in which very similar results to those he had put on the board were given. He (Mr. Richmond) did not think that the experiments recorded in the paper he referred to treated of the influence of temperature; but they showed that the error increased with the bulk of the solution, and a correction formula was given. He would like to know if Mr. Young had consulted that paper, and, if so, how the results compared with his own.

The Chairman asked Mr. Young if he had tried the method which he (Dr. Dupré) had proposed some years ago,† of conducting the process, with potassium chromate as an indicator, in a porcelain dish, and viewing the sodium chloride solution through a solution of potassium chromate solution of approximately the same tint as the contents of the dish. The first faint appearance of a red colouration is in this way rendered much more distinct. The marked increase of delicacy in using the process by gaslight had led him to adopt this method in daylight.

Mr. Young, in reply, said he had not seen the paper referred to by Mr. Richmond. He had gone through a good many of the later Journals of the Chemical Society to search for any paper on the subject, but had failed to find any. He had asked the

* The Analyst, vol. xiv., 229. † The Analyst, vol. v., 123.

opinion of two or three well-known chemists—among them Professor Ramsay—and they had said that the solubility of chromate of silver was utterly unknown to them. He had found difficulty in making theoretically correct standard solutions. He had made up the solution for this work with the purest silver nitrate he could get, and had found a great difficulty in standardizing it. He thought that the error was due to the solubility of the chromate of silver. He had not used a vessel filled with a solution of potassium chromate such as Dr. Dupré had described. He quite agreed with Dr. Dupré that by using such an arrangement the difficulty in accurately observing the change of colour would be got over.

Dr. Dupré asked Mr. Young if he had standardized against salt.

Mr. Young replied that he had found it impossible to standardize accurately. He titrated his silver nitrate solution against a known quantity of sodium chloride, and the silver solution appeared to be very slightly weak. As there could be no doubt about the purity of the silver nitrate, he had adopted this solution as correct. The strength of the solution would not affect the differences in the results.

Mr. Bodmer asked the author, Mr. Young, whether the nitrate of silver used was the fused or the crystallized nitrate.

Mr. Young replied that he had used crystallized silver nitrate, and that he had re-crystallized it twice himself.

Mr. Bodmer stated that if ordinary commercial common salt were used the same results were not arrived at as when rock salt was used. Absolutely correct results could be obtained by using fused nitrate of silver.

THE LEFFMANN-BEAM METHOD FOR THE ESTIMATION OF FAT IN MILK.—Part II.

By H. Droop Richmond.

(*Read at the Meeting, December 7th*, 1892.)

In the former portion of this paper (The Analyst, xvii. 144), I showed that the volume of fat depended on an extension of Henry's law, and I showed that this held with considerable exactitude; my experiments, however, were conducted using a set of bottles made by Messrs. Müller and Co., which were graduated so that 86 divisions equalled 1·475 c.c., and occupied a length of 5·3 centimetres, and the capacity varied from 28·5 c.c. to 30·5 c.c.; this variation I found to make no difference. I have since experimented with a new series of bottles, obtained from Messrs. Baird and Tatlock; these were graduated so that 100 divisions equalled 1·875 c.c., and occupied a length of 5·53 centimetres, and the capacity was 29 c.c.

With these latter I find that the same law does not hold; these bottles had been graduated on the assumption that, using the proportions of milk, fusel mixture, and acid

given by me, 80 divisions (= $\frac{86}{1\cdot07}$) should occupy 1·5 c.c.; this would have been substantially correct had the extension of Henry's law given by me been applicable to conditions other than those under which I worked, but it evidently is not. Before entering into the theory of this subject, I propose to give a few results with the bottles received from Messrs. Baird and Tatlock, the English agents for this machine. I have found it necessary to multiply the readings by 1·08 in order to obtain the actual percentage of fat in the milk; with this correction the results are as accurate and reliable as I had previously found. In fact, the more I have used this method the more I have been convinced of its accuracy, and this, combined with the speed and ease of working, places it in the front rank of practical analytical methods.

SERIES IV.

No.	Amount of Fat.	L. and B.	L. and B. corrected.	Error.
68	3·98	3·7	4·00	+ ·02
69	3·65	3·35	3·62	—·03
70	4·64	4·2	4·54	—·10
71	2·93	2·7	2·92	—·01
72	4·05	3·75	4·05	—
73	1·96	1·8	1·94	—·02
74	3·87	3·6	3·89	+·02
75	4·24	3·9	4·21	—·03
76	3·62	3·45	3·73	+·11
77	3·60	3·3	3·56	—·04
78	3·56	3·3	3·56	—
79	3·58	3·3	3·56	—·02

These results are but a few of those actually obtained, and are not in any way picked, but are extracted at random from my note books.

To turn to the theory of the subject; it was at first startling to find that a comparatively small variation of the conditions, *i.e.*, the size of bottles, should make such a large variation in the results, indicating that the theoretical considerations I had adopted were not wholly correct. I therefore, in order to test these, made the following experiment:—

80. About 15 grams of the fatty layers obtained in various estimations were shaken with boiling water until the water was practically neutral; this was filtered and neutralized with decinormal barium hydrate, a large precipitate separating; the solution was filtered and washed with hot water; the filtrate was found only to contain small quantities of barium, and a very notable proportion of chlorine, so that the presence of considerable quantities of amyl-hydrogen sulphate in the fatty layer was disproved, and my assumption that the layer measured contained this, was not correct.

Were there nothing to increase the volume of the fat the amount equalling 8·0 per cent. fat would be 1·35 c.c., while in B. and T.'s bottles it is 1·39 c.c., and in M. and Co.'s it is 1·475. To what is the excess due?

While drawing off the fatty layers I was struck by the way in which the fat adhered to the sides of the dry tube used to remove it, while it did not adhere to the neck of the flask. This suggested to me that there was a layer of aqueous liquid between the fatty layer and the sides of the neck, and consequently that the actual diameter of the fatty layer was less than the internal diameter of the neck. To enter deeper into this subject, which involves the consideration of difference of surface tension between three substances—the fatty layer, the acid mixture, and the glass, and of capillary attraction, is impossible with the meagre data that exist; the very fact of the fat rising so completely and rapidly shows that the differences of surface tension must be enormous, and play a very important part in the solution of this question.

I must, therefore, renounce the theory that amyl-hydrogen sulphate is dissolved in the fat, and that this accounts for the increase of volume. It is not, therefore, necessary to employ the extension of Henry's law given in Part I. My results emphasise the fact that this method is essentially a laboratory method; the actual factor connecting readings and percentages of fat must be worked out for each bottle, and it will not do to trust to the graduations, even if the absolute volume is checked. As I find that my two series of bottles agree among themselves, I have hoped that by adopting fixed dimensions, bottles can be graduated in quantity to read percentages; but it is possible that one quality of glass must be adhered to. There is some evidence that the quality of glass does not make very much difference, as I have obtained substantially the same results with B. and T.'s bottles as with a bottle made by myself, using the broken neck of one of the bottles received from Messrs. Leffmann and Beam. The diameter of the two necks differs only by 0·02 mm.

I prefer the size of neck adopted by Müller & Co. of internal diameter 0·5955 c.m. as giving larger divisions and more exact readings, and I would suggest that bottles should be made with necks of this diameter, and graduated so that 80 divisions = 1·475 c.c. and occupy a length of 5·3 centimetres, that the volume should be 29 c.c., and that the same glass should be used for all.

Analysis of Cream.

I have made a number of experiments on the analysis of cream, and find that the measurement of the cream in a 10 c.c. specific gravity bottle is not sufficiently exact for creams containing more than 30 per cent. of fat. It is necessary to weigh it; for this purpose I measure a definite volume of cream, 3 c.c. if using B. and T.'s bottles, and 2·5 c.c. if using M. & Co.'s, into the previously-weighed bottles, and weigh it. I then add 12·2 or 12·7 c.c. of water respectively, and fusel mixture and sulphuric acid as usual.

The reading is multiplied by 15·74 for B. and T.'s bottles, and 14·11 ($= \frac{15.4}{1.07}$) for M. and Co.'s bottles, and divided by the weight of cream taken; the results are only accurate to about 1 per cent.; this, however, is sufficient for the ordinary purposes of cream control.

No.		Fat Gravimetric.	L. & B.	Error.
81.	...	50·9 W.S.	50·5	— ·4
82.	...	42·8 V.	43·0	+ ·2
83.	...	25·6 W.S.	24·3	— 1·3
84.	...	48·9 W.S.	49·7	+ ·8
85.	...	44·3 V.	44·1	— ·2
86.	...	45·9 W.S.	46·2	+ ·3
87.	...	47·9 V.	48·0	+ ·1
88.	...	46·3 V.	47·3	+ 1·0

The above were obtained with Müller & Co.'s bottles.

No.		Fat Gravimetric.	L. and B.	Error.
89.	...	40·1 W.	39·2	— ·9
90.	...	41·9 W.	41·2	—·7
91.	...	47·0 W.	48·5	+1·5
92.	...	37·9 V.	38·4	+·5
93.	...	45·0 V.	44·5	—·5

These were obtained using B. and T.'s bottles.

Nos. 92 and 93 were each done in triplicate to test the obtainable accuracy.

The results were:—

No.				
92.	...	38·8	38·1	38·3
93.	...	45·2	43·9	44·4

I have found that with M. and Co.'s bottles, the weight of fat deduced from the results obtained with milk, can be used when analysing cream, while with B. and T.'s bottles it is necessary to divide it by 1·06.

Sundry Experiments on Milk.

A few experiments were made using both sets of bottles:—

No.		M. and Co.	M. & Co. $\times \frac{1}{1.07}$	B. and T.	B. & T. × 1.08
94.	...	4·25	3·98	3·7	4·00
95.	...	4·36	4·03	3·75	4·05
96.	...	3·8	3·55	3·3	3·56

Another set of experiments was made comparing the results obtained by using the fusel oil mixed with sulphuric acid, with those obtained in the ordinary way.

No.			Sulphuric Acid.			Hydrochloric Acid.
97.	...	...	3·56	...	...	3·56
98.	...	...	3·56	...	...	3·56
99.	...	...	3·50	...	...	3·56

In order to see how the process agreed with itself when the results were repeated, six bottles were all filled with the same milk (B. and T.'s bottles being used); the results were:—

No.												
100.	...	Reading	3·8	—	3·8	—	3·75	—	3·8	—	3·8	— 3·8
		= Fat	4·10	—	4·10	—	4·05	—	4·10	—	4·10	— 4·10

Conclusions.

The results recorded in this paper show—

(i.) That this method is very accurate and reliable when once the bottles are graduated.

(ii.) That it is necessary to standardize each bottle before use; this shows that it is unsuited for use outside the laboratory, unless the bottles are first tested.

(iii.) That creams may be roughly analysed by its means; the analysis of cream by this process is not so satisfactory as the analysis of milk.

(iv.) That the use of hydrochloric acid is not absolutely necessary; the fusel oil may be mixed direct with the sulphuric acid; the separation of the fat is not quite so sharp in this case, and, on this account, the omission of the hydrochloric acid is not to be recommended.

The principle of this method appears to be quite distinct from that of any other process of fat estimation. Besides the principle of dissolving the albuminoid employed in the methods of Hoppe-Seyler, Soxhlet, Röse, Planchon, Cochran, and last, but not least, Werner-Schmid, and the application of centrifugal force, and measuring of the fat due to Babcock, there is the use of fusel oil to promote by a large difference of surface-tension the easy separation of the fat. This places the Leffmann-Beam method in a distinct category, and renders it the most perfect of volumetric processes for the estimation of fat in milk.

I have to record my thanks to Mr. A. Schnepel for help in this investigation.

The Volatile Fatty Acids of Butter. H. Kreit. (*Schweiz. Wochenschr. Chem. Pharm.*, 1892, xxx., 449, through *Chem. Zeit.*)—The author confirms the statement of M. Schrodt and O. Henzold that the Reichert-Meissl figure is dependent on the period of lactation. Of 75 samples examined during a year, the Reichert-Meissl figure ranged from 21·1 to 34·4; 4 per cent. were under 22; 24 per cent. between 22 and 24; 32 per

cent. between 24 and 26; 23 per cent. between 26 and 30; and 17 per cent. over 30. All the samples that gave a figure higher than 30 were from cows which had recently calved. B. B.

Boric Acid as a Normal Constituent of Beer and a Natural Constituent of Hops. J. Brand. (*Zeits. gesammt. Brauw.*, 1892, xv., 427, through *Chem. Zeit.*)—The method of detecting the presence of boric acid used by the author was that depending upon the well-known reaction with turmeric paper. The process adopted consisted in making the beer to be examined slightly alkaline, evaporating it to dryness and incinerating, extracting the ash with water, evaporating the solution nearly to dryness, making faintly acid with hydrochloric acid, and heating on the water-bath after the immersion of a strip of turmeric paper. Beer containing only 0·1 per cent. of boric acid gave a definite reaction under these conditions, and the turmeric paper afterwards afforded the characteristic green coloration with alkali. Pure beer, to which, according to the author, no boric acid had been added in the course of manufacture, also gave the reaction. Beer of various origins was examined, the samples being Munich, Bavarian, German, Austrian, and Brazilian, and boric acid was found in all. In order to put the presence of boric acid beyond doubt, Gooch's method, depending on the volatility of methyl borate, was used, The ash from 200 c.c. of beer was distilled with pure methyl alcohol and sulphuric acid, and the distillate received in ammonium carbonate solution. The liquid in the receiver was evaporated to a small bulk, supersaturated with hydrochloric acid, a strip of turmeric paper dipped in, and the whole taken to dryness. Boric acid was recognised in all cases. The origin of the boric acid now engaged attention. Various samples of the ash of malt and barley were examined, but no trace of boric acid found. The ash of hops, however, gave marked indications of boric acid. Eighteen different kinds of hops were examined, 5 grams of each being incinerated without the addition of alkali, and the ash distilled with methyl alcohol and sulphuric acid, and in all cases turmeric paper was coloured and gave the usual subsequent reaction with alkali. It therefore appears that the boric acid normally present in beer is due to that existing as a natural constituent of hops. Hops, both cultivated and wild, plucked direct from the growing plant, were found to contain boric acid, which was detected in the leaves, stems, and tendrils, as well as in the actual flower. B. B.

Ammonium Thiocyanate in Manures. P. L. Jumeau. (*J. Pharm. Chim.*, 1893, xxvii., 190, through *Chem. Zeit.*)—The author, in examining a sample of nitrogenous manure and one of ammonium sulphate, which proved deleterious to the crops for which they were used, found that they contained ammonium thiocyanate, which was the cause of the evil effects observed. The sample of ammonium sulphate had the following composition:—Moisture, 10·71; ammonium sulphate, 67·84; ammonium thiocyanate, 9·39;

sodium sulphate, 9·24; potassium sulphate, 0·98; calcium sulphate, 0·68; ferric oxide, 0·30; silica, 0·08 parts per cent. respectively, and traces of chlorine and magnesia. The determination of the thiocyanate was effected by dissolving 5 grms. of the sample in 200 c.c. of water, filtering, and determining the sulphate in 10 c.c. of the filtrate. Another portion of 10 c.c. was acidified with hydrochloric acid, the thiocyanate oxidised with potassium permanganate, and the resulting sulphate determined by precipitation with barium chloride in the usual way. The difference between the two determinations of sulphate gave a measure of the amount of thiocyanate present. An alternative method, which is available in the absence of chlorides, consists simply in titrating the aqueous solution with silver nitrate in the presence of a small quantity of nitric acid. The addition of a little ferric chloride serves to provide an indicator, the reaction being completed when the liquid is decolourised. The procedure can, of course, be modified by adding an excess of silver and titrating back with a standard solution of ammonium thiocyanate. Should chlorides and cyanides be present, the following plan may be adopted:—A standard solution of ammonium thiocyanate is prepared, its value being accurately determined by means of titration with silver nitrate or by oxidising with permanganate and weighing the resulting sulphate as barium sulphate. It should have a strength of about 8 grammes per litre. A solution of potassium permanganate is then prepared, containing about 10 grammes per litre, and used to titrate the standard solution of ammonium thiocyanate, of which 10 c.c. are taken, diluted to 100 c.c., and acidified with 10 c.c. of pure sulphuric acid. The appearance of a permanent pink colour indicates the end of the reaction with perfect sharpness. The same process is then used for the sample to be analysed, of which about 0·5 grammes is taken. It is alleged that the permanganate has no action on cyanides and chlorides under these conditions, a statement that seems in need of confirmation. B. B.

Methyl and Ethyl Alcohols. C. A. Lobry de Bruyn. (*Bericht. deutsch. Chem. Gesell.*, 1892, xxvi., 268–274.)—The author shows that the solubilities of ammonia, and of potassium cyanide, potassium iodide, mercuric cyanide, and hydroxylamine hydrochloride are greater in methyl than in ethyl alcohol. Colourless solutions of sodium methylate, containing 0·1 to 0·2 gramme Na per c.c. can be prepared from methyl alcohol, care being taken that during the addition of the sodium they are kept cool. Such solutions, when kept out of contact with cork and similar organic substances, do not become brown, and when preserved in a dry atmosphere, free from carbonic anhydride, do not alter in titre. Solutions, either of sodium or soda, in methyl alcohol are, therefore, to be preferred to those in ethyl alcohol for such uses as that of the titration of free fatty acids in fats. Sodium ethylate is much more readily oxidised than sodium methylate; consequently, the former possesses stronger reducing properties than the latter. Spongy platinum and potassium permanganate act on methyl alcohol much less readily than on

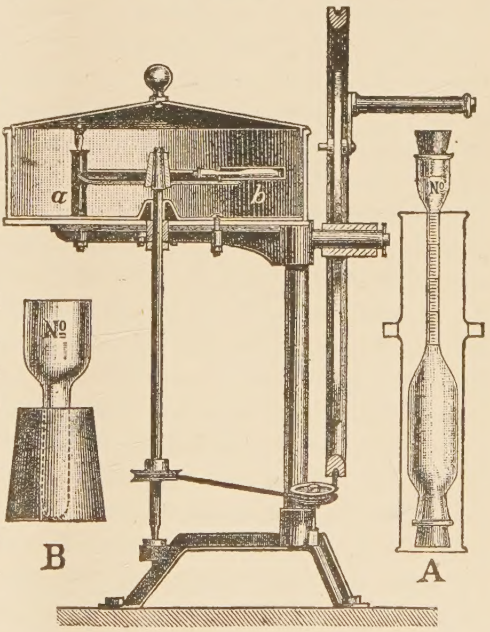
ethyl alcohol, and dry bleaching powder, which is without action on the former liquid at the ordinary temperature, spontaneously interacts with the latter in from 7 to 10 minutes. When chlorine is passed into either of the alcohols a reaction sets in much sooner in the case of ethyl than in that of methyl alcohol. The difference in reactive power is still more marked in the case of bromine. A mixture of bromine and methyl alcohol, containing 61·7 per cent. of bromine was still found to contain 57 per cent. of bromine after 78 days. Iodine dissolves in the two pure alcohols to about the same extent; but whereas a saturated solution of iodine in methyl alcohol does not alter its titre after many weeks, one in ethyl alcohol gradually becomes weaker. The apparent larger solubility of iodine when it is triturated with ethyl alcohol than when it is left at rest with this liquid, is due to the fact that the reaction products are better solvents for iodine than is the pure alcohol.

Methyl alcohol is somewhat more hygroscopic than ethyl alcohol.

A. R. L.

Acido-butyrometry. N. Gerber. (*L'Industrie Laitière*, 1892, 50, 397; 52, 413; 1893, 1, 1; 6, 43.) The author, dissatisfied with the methods for the rapid estimation of fat in dairy products, has devised a new method for the purpose. The proposed method is essentially the same as that due to Leffmann and Beam, insomuch that amyl alcohol is employed to promote the easy separation of the fat from the aqueous portion.

The centrifugal machine used is shown in the figure, in which the bottles are also figured. These are of two kinds, for liquids and for solids, such as cheese and butter. The one shown in the figure is that used for solids; that used for liquids differs from the other by having the top closed. The estimation of fat is performed as follows:—



Milk.—10 c.c. of milk are measured into the bottle, 1 c.c. of amyl alcohol is added, and then 10 c.c. of strong sulphuric acid; a cork is inserted into the open end, the mixture shaken and whirled in the machine for 2 to 2½ minutes. During the whirling the outer case of the machine should contain water kept warm by a spirit-lamp; the machine is allowed to run down, and the percentage of fat is read directly.

Cream, butter, cream-cheese (*fromage à pâte molle*).—1 c.c. is measured in the cup B (see figure), which is fixed firmly into a cork, which goes into the lower end of the

bottle; 8 c.c. of water are added, and amyl alcohol and acid as before. The reading must, in this case, be multiplied by 10 to obtain percentages. If great precision is required, or if ordinary cheeses are to be analysed, 1 gramme should be weighed instead of 1 c.c. being measured; but the author is of opinion that measurement is sufficiently exact in practice. H. D. R.

Rapid Determination of Crude Fibre by Means of the Centrifugal Separator. W. Thörner. (*Chem. Zeit.*, 1893, xvii. 394 to 395.)—The tedious process of determining crude fibre in food stuffs is much expedited by the aid of the centrifugal methods already described by the author for various other analytical separations (The Analyst, 1891, 210). The vessel used is a tube with a collar or ridge near the mouth, so as to allow it to hang in the water-bath during the solution of the material to be analysed. It has a capacity of 50 c.c., and the quantity of substance taken for analysis is 1 gramme. Should the food stuff, in which the crude fibre is to be determined, contain any considerable quantity of fat, it is first treated with 20 c.c. of ether, and the tube containing it rotated for one or two minutes in the machine, after which the ether can be poured off without fear of loss, and the operation repeated once or twice until the fat is completely removed. The ether is then driven off the residue by heating in the water-bath, 30 c.c. of hot water added, and the heating continued for about 10 minutes, the contents of the tube being stirred with a glass rod provided with a flattened knob at the end. 10 c.c of dilute sulphuric acid are added (50 c.c. of the strong acid made up to a litre), and the heating and stirring continued for another 30 minutes. The rod is withdrawn, the tube put in the centrifugal machine and rotated therein at a speed of 2,000 revolutions per minute, for 3 or 4 minutes. The insoluble matter is nearly completely separated in a compact form at the bottom of the tube by this treatment, and the more or less turbid liquid is poured off through a weighed filter sufficiently large to hold the whole of the contents of the tube. The tube is then refilled with about 40 c.c. of hot distilled water, and the stirring with the glass rod repeated for a period of 10 to 15 minutes, the tube being meanwhile suspended in the water-bath. The clear liquid is decanted through the filter and the washing repeated. The residue in the tube is then treated with 30 c.c. of hot water and 10 c.c. of a solution of caustic potash containing 50 grammes of KOH per litre, heated in the water-bath, and stirred as before for half-an-hour. Washing with water, as already described for the treatment with acid, is then performed, and the crude fibre thrown on to the filter, washed in succession with alcohol and ether, dried and weighed in the usual way. The whole determination can be completed in three or four hours, and with a moderate-sized centrifugal machine 8 determinations can be carried on simultaneously.

B. B.

REVIEWS.

DIE CHEMISCHEN UNTERSCHIEDE ZWISCHEN KUH UND FRAUENMILCH UND DIE MITTEL ZU IHRER AUSGLEICHUNG. (The chemical differences between human and cow's milk and the means of compensating them.) F. Soxhlet. Pamphlet, J. F. Lehmann, Munich, 1893. Price 60 pf. (7½*d.*)

The author shows that the differences beeween the two milks are :—

1. The different behaviour of the casein when coagulated. 2. The different amount of salts. 3. The difference in the amount of total food-material and in the relative proportion of the constituents.

As dilution modifies the behaviour of the casein of cow's milk towards rennet-enzymes and brings it nearer to the casein of human milk, he proposes to make an artificial human milk by diluting cow's milk with half its volume of a 12·3* per cent. solution of milk sugar.

The composition of human milk, cow's milk, and artificial milk are said to be :—

	Human	Cow	Artificial
Water	87·41	87.17	85·30
Albuminoids	2·29	3·55	2·37
Fat	3·78	3·69	2·46
Milk Sugar..	6·21	4·88	9·40
Ash	0·31	0·71	0·47

The seeming excess of milk sugar is due to an allowance of 2·43 parts of sugar for each 1 part deficient of fat, after the observations of Rubners ; to meet the deficiency of 1·32 of fat, 3·19 of milk sugar has been added. Professor Soxhlet takes no notice of Frankland's principle of preparing artificial human milk. He condemns the addition of cream to make up the deficiency of fat, on account of there being no method of estimating the fat in the materials used ; he considers his preparation to be capable of giving a milk of constant composition, and even states that the milk of normally fed cows is of constant known composition. How erroneous his view is will be evident to our readers ; the rapid and accurate methods of fat estimation and the variations in the composition of cow's milk are too well known to need a reference here. There seems no justification for, to quote Soxhlet's own words, "adopting the less correct plan of making up the deficiency of fat by some substance as nearly as possible equivalent to fat," and to substitute a rough estimate of the food-value of milk sugar for an accurate estimation of fat.

* *Note by Reviewer.*—Though Soxhlet directs the use of half the volume of a 12·3 per cent. solution of milk sugar, a simple calculation shows that a solution containing 18·44 per cent. of anhydrous milk sugar, or 19·4 per cent. of hydrated sugar must be used to obtain the composition given by him ; such a solution is nearly saturated and can only be prepared by long agitation in the cold or by means of heat. There are other arithmetical errors.—H. D. R.

Professor Soxhlet reviews the evidence available as to whether milk sugar can be replaced in infants' foods by other bodies of a similar nature, *e.g.*, cane sugar, dextrose, and maltose, and very properly comes to the conclusion that it cannot be so replaced; milk sugar has dietetic properties not possessed by any other available sugar.

With the exception of the portion alluded to, to which objection may be taken, this is a most valuable contribution to our knowledge of milk and the pamphlet will well repay perusal. H. D. R.

Galenic Pharmacy. By R. A. Cripps, F.I.C., Pharmaceutical Chemist. (London, J. & A. Churchill.)

This work ably fills a void which has hitherto existed in modern literature, for, as the author says in his preface, "while works on Chemistry, Botany, and Physics have amply provided the student with instruction in these sciences, he has been left almost without help in their application to Pharmacy."

After an introduction, the author devotes a chapter to preliminary chemical notes, in which he explains, somewhat unnecessarily we think, the use of symbols and formulæ and gives definitions of elements, compounds, atoms, molecules, &c. The chapter which follows on the structure of plants is more in place and the descriptions are concise and clear.

The author then proceeds to discuss Pharmacy proper, describing in detail, with the aid of excellent wood-cuts, the various processes required to be carried out in a Pharmaceutical Laboratory. Trituration, Solution, Filtration, Evaporation, Distillation, &c., are all well and fully described. Where definite chemical reactions occur the formulæ are given, and the various tests for the preparations are detailed.

The work, which seems admirably adapted for its intended purpose, concludes with a number of useful definitions, and an appendix containing Tables of Temperature, Specific Gravity of Spirit, and other useful data.

Throughout the book there is abundant evidence of practical knowledge of the subjects under treatment, and one is forcibly struck with the contrast between the author's able descriptions and the imperfections of that monument of slovenly and perfunctory work, the British Pharmacopœia.

The work is a credit to the author, who is both an able Chemist and a well-known Pharmacist, and will be found very useful to Public Analysts, who are rather apt to give less attention to that portion of their duties bearing on Pharmacy, and especially Galenic Preparations, than is desirable.

The whole work covers some 320 pages and should find a place in the laboratory of every practising analyst. A. H. A.